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Available online 5 April 2009



doi:10.1016/j.ejvs.2009.01.018

## Response to Comment on ''Screening for Abdominal Aortic Aneurysm and Overall Mortality in Men''

Dr. Koelemay must be acknowledged for his careful review of the meta-analysis, which has revealed a citation error in the manuscript, for which we apologise. The data used in the original and revised long-term meta-analyses are not from the preliminary 10-year report from the Viborg study as cited,[1] but from the complete 7-year report,[2] as for the MASS trial.[3] If the preliminary 10-year results are used in the long-term analysis, we agree that the upper 95% C.I. limit for the odds ratio hits 1.00.

For interpretation, one has to remember the effect of screening and the data used in the calculation of odds ratios. The effect is a delay of death but in the end, we are all going to die. Thus, the most relevant statistics to use are survival analyses. However, this would require merging of results from all the randomised trials. This has been attempted but without success. Consequently, we can only use meta-analysis to address this question. However, calculation of the pooled odds ratio is based upon the number of deaths in the invited group versus the control group and ultimately the numbers will be equal in the two groups, and the OR will become 1.00. The modified calculation seems just to be a manifestation of that progression.

The existing meta-analysis still shows that screening reduces overall mortality, and the risk association would probably have been even stronger if seven-year results from the Chichester and Australian Studies had been available instead of after 15 and 12 years, respectively.

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Available online 31 March 2009



doi:10.1016/j.ejvs.2009.03.003

## Comment on: ''Negative Pressure Wound Therapy: a Systematic Review of Effectiveness and Safety'', P. Vikatmaa, V. Juutilainen, P. Kuukasjärvi, A. Malmivaara, *Eur J Vasc Endovasc Surg* 2008;36(4):438–48

*Sir,*

We were surprised by the publication of this systematic review. However valuable, the topic and contents are the same as was published earlier in the British Journal of Surgery and the Cochrane Library.[1,2] Publication of a Cochrane systematic review also in a paper-based medical journal has been accepted to enlarge the readership to those who have limited access to the Cochrane Library.

The authors of the present paper include virtually the same trials and reach a similar conclusion. They even refer to the previous systematic review, but do not (and obviously cannot) indicate what is new in their work. Although this may be deductable to a juxtaposition of nearly simultaneous publications, it has led to redundancy in the surgical literature.

Although we understand the interest of editors of medical journals to include systematic reviews as potentially high-referenced sources of the state-of-the-art for their readers, they should avoid even the semblance of double publication. For this purpose, a section with summaries of interesting work published in other journals would be fair to the original authors. The Journal of Vascular Surgery has done so, but without referring to the original authors.[3] Again, such a publication appears neither original nor ethical.

In preparing their manuscript, authors should define what is known and motivate what is new in what they want to

DOI of original articles: 10.1016/j.ejvs.2008.03.006; 10.1016/j.ejvs.2009.02.018.

DOI of original article: 10.1016/j.ejvs.2008.06.010.